

*STILL NO EVIDENCE FOR TEMPORALLY  
EXTENDED SHOCK-FREQUENCY REDUCTION AS  
A REINFORCER*

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There is no consensus and very little overlap in the criticisms of my target article. Because the primary consequences of avoidance behavior are by definition alterations in the distribution of shocks in time, any theory about the reinforcement of such behavior necessarily must begin with that dimension. However, the safety-signal version of two-process theory calls on positively and negatively correlated stimuli, including the responses themselves serving as stimuli, to transmit the effects of those alterations to the relevant behavior. Meanwhile, the Herrnstein–Hineline single-process theory hypothesizes an additional source of reinforcement: a direct effect of reduction in the density of shock over some extended period of time. I can find no data that selectively support that hypothesis.

*Key words:* safety signal, response as stimulus, two-process theory, correlated stimuli, shock-density reduction, avoidance, electric shock

If there had been a significant gap in the evidence or the logic that I presented in my target article (Dinsmoor, 2001), it seems likely that several of the well-qualified behavior analysts who have written commentaries would have spotted it and set me straight. However, I detect no consensus in the criticisms they advance and very little overlap. This suggests that there is no major, obvious flaw in my argument, but at the same time it makes it difficult to write a compact reply. I will deal first with issues on which more than one commentator appears to be advancing a similar criticism, and I will then review each of the commentaries individually to make sure I have not left any significant objection unanswered. The reader should be aware, however, that there are also instances in which views have been imputed to me that I do not accept.

#### RESPONSES AS STIMULI

The largest overlap I detect comes from three commentaries that express varying degrees of concern over my treatment of responses as stimuli. “Proprioceptive and kinesthetic feedback . . . [are] rarely observed,” says Baum, “but Dinsmoor has a solution to that: Assume the stimuli and observe the re-

sponse” (Baum, 2001, p. 339). Branch, too, is puzzled by my claim “that the stimuli associated with responses are observable” (Branch, 2001, p. 353). That description of my views is not quite correct, but part of the fault may be mine for the somewhat loose use elsewhere in my article of words like *stimulus* and *stimuli*. It is difficult to discuss stimuli in impeccably scientific terms without becoming tied up in verbal knots. Properly speaking, however, it is the physical event or energy impinging on the organism that constitutes the stimulus, not the reaction of the animal’s sensory apparatus or nervous system to that event, let alone a phenomenological experience. As for Branch’s concern about the fading of stimuli with time, we have only the general evidence: As another commentator put it, “Stimuli and responses affect present behavior less and less as they recede into the past” (Baum, 2001, p. 340).

As observers we can make *direct* contact with exteroceptive stimuli that affect another organism in ways that we have strong reasons to believe are very similar to the contact made by the experimental organism, but we only make *indirect* contact with kinesthetic stimuli that affect that organism. (Michael & Clark, 2001, p. 354)

It is quite true that the nature of the contact differs between subject and observer, but in my opinion, essentially the same problem is raised when lights, tones, visual or auditory patterns, shocks, or flavors are used as labo-

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ratory stimuli. These environmental events are detected by subject and experimenter from different vantage points and using sensory apparatus that differs from moment to moment, individual to individual, and species to species. For these two individuals, the sensory experience must rarely, if ever, be the same. The pattern on the key, for example, extends over a much larger visual angle from the position of the pigeon immediately in front of it than from the position of the human observer at a somewhat greater distance; the experimenter relies on a meter reading rather than on his or her cutaneous receptors to detect the flow of electric current; individual humans differ in their gustatory sensitivity to various chemical substances, and different species presumably do the same; different sensory adaptations may have occurred in experimenter and subject; and the human observer may not detect a high-frequency sound that is discriminable to the rat or ultraviolet wavelengths or magnetic fields that are discriminable to the pigeon. As experimental analysts of behavior, we have to persevere despite such difficulties.

Perhaps I should add that proprioceptive stimuli are not the only form of sensory feedback from the subject's behavior. Visual and auditory changes reflecting the location of the animal in the experimental apparatus, the sound of the relay that provides auditory feedback, tactile stimulation from the lever, the impact of the beak on the key, and so on, must also play a role.

#### SAME PHYSICAL CONTINUUM

Both Baum and Sidman note that in my target article I often make reference to the frequency or density with which shock is delivered. "Dinsmoor is still saying that shock frequency is critical" (Sidman, 2001, p. 337). "Dinsmoor cannot explain the avoidance without reference to shock-rate reduction" (Baum, 2001, p. 339). Obviously, some clarification is needed. Two-factor theory and the shock-frequency-reduction hypothesis of reinforcement do have something in common: Of necessity, they both refer to portions of the same physical continuum, the number of shocks that are delivered within a certain interval of time. In no way, however, does this fact compromise my analysis.

The difference between the two approaches does not lie in whether or not they refer to a particular physical continuum or in what terminology they use to refer to that continuum: It lies in the portion they use of that continuum and the way they use that continuum. Two-factor theory attends to the temporally proximate relation between the shocks and otherwise innocuous stimuli and to the temporally proximate relation between the subject's behavior and those stimuli. There are two steps, mediated by the presence of the stimuli, and the mediation provides for an intimate contingency or correlation that enables the shock schedule to act selectively, like the traditional schedules of reinforcement, on a specified form of behavior. Regardless of whether the single-process (frequency reduction) account is advanced as an alternative or simply as an addition to the earlier two-process account, the feature that distinguishes it from its predecessor is its direct reliance on the inverse correlation, over some period of time extending beyond the traditional Skinnerian contingency, between the number of responses and the number of shocks (e.g., Baum, 1973; Herrnstein, 1969; Hineline, 1981; Sidman, 1966).

A very similar issue arises with the reciprocal relation between a delay of shock, abstractly considered, and a reduction in its frequency. It is true that these are also alternative metrics for the same physical continuum. But again the difference between the two bases for the reinforcement of avoidance is not merely one of vocabulary. When we think in terms of delay, we look at short-term functions. We accept the relevance of temporal proximity. When we think in terms of frequency reduction over a more extended period (molar principle of reinforcement), we are ignoring the role of temporal proximity. However, as indicated in my target article, the sharply diminishing effectiveness of reinforcing and punishing consequences as a function of the length of time by which they follow the response indicates that temporal proximity is indeed an important parameter.

#### CONTIGUITY VERSUS CORRELATION

Both Hineline (2001) and Baum (2001) cite Rescorla's (1967) treatment of differenc-

es between the frequency of shock in the presence of the conditional stimulus and its frequency in the absence of that stimulus. Apparently they both believe that Rescorla's characterization of those differences as a measure of correlation obviates the need to consider temporal proximity as a factor in Pavlovian conditioning. But temporal proximity and correlation are not incompatible concepts. Note that Rescorla began his analysis by counting the number of instances of strict contiguity (i.e., actual temporal overlap) under either stimulus condition—his raw data, so to speak—and only subsequently calculated a summary statistic based on the difference between the two frequencies. My comparisons of the incidence of shock in the presence or the absence of some specific exteroceptive stimulus or just before and just after each occurrence of the target response seem to me to be entirely in keeping with Rescorla's treatment. However, not all relations that can be labeled *contingency* or *correlation* share the same structure and content, and I think that the longer term correlations to which Baum and Hineline make their appeal (shock-frequency reduction) are entirely different from the one discussed by Rescorla. I question the relevance of their comparison. For further elaboration of Rescorla's thinking, I would also call their attention to Rescorla and Wagner (1972).

#### THE SESSION-SHORTENING EXPERIMENT

Both Baum (2001) and Branch (2001) defend the validity of the session-shortening procedure used by Mellitz, Hineline, Whitehouse, and Laurence (1983) as evidence for the reinforcing effect of temporally remote consequences. (Hineline, 2001, describes my analysis as molar.) In my description of that experiment, I granted that the relation between pressing the nonpreferred lever and the ending of the experimental session was response independent, but I see no evidence in the Mellitz et al. procedure section of the 20-s delay cited by Branch or the 2-min delay between response and consequence suggested by Baum. The 20 s was a parameter of the baseline shock-postponing schedules, and the 2-min delay related only to the contingency for termination of the session, not to the ter-

mination itself. Noncontingent reinforcers have been known to maintain substantial rates of pecking or pressing, and in this case a reinforcer I would assume to be quite powerful—removal from the experimental chamber, where many shocks had been received, and return to the home cage, where many feedings had been provided—was imposed on an existing rate of responding that was already sufficient to ensure many instances of temporal proximity. Although the effect of ending the session as a reinforcing consequence was sometimes large, it was not entirely consistent, and that is just the result that might be expected under a noncontingent schedule of reinforcement. In evaluating this experiment, I do not think the burden of proof lies with the critic but with the original experimenters and those who support their interpretation of the findings. Also, I had already noted in my target article that reinforcement of behavior by a temporally distant consequence—in itself an unusual result in the conditioning literature—does not necessarily lend support to the more specific hypothesis that a decline in the frequency of aversive stimulation over an extended period of time engenders an increase in the rate of responding (shock-frequency reduction).

#### SIDMAN

Sidman (2001) recognizes the positive contributions—as distinct from my critique of shock-frequency reduction—of my target article to an understanding of the literature on avoidance. He reviews the evolution of what might be termed nonhypothetical or behavior-analytic two-factor theory since its original formulation by Schoenfeld (1950) and concludes that the addition of the concept of the safety signal “restores a legitimate explanatory status to response-produced stimulation” (Sidman, 2001, p. 336). It also “solves the problem of the sometimes rapid learning of free-operant avoidance” (p. 336). “With the response-produced safe period, [Dinsmoor] has added a powerful and perhaps more widely applicable explanatory principle” (p. 338).

In addition, however, Sidman (2001) raises a question. “Why should a stimulus that is negatively correlated with shock become reinforcing?” (p. 337). Some theorists may wish

to refer to Pavlovian inhibitory conditioning as an answer to that question, but I do not see the need for any explanation beyond the descriptive level. "Without danger," Sidman continues, "safety has no meaning" (p. 337). It is true that neither positive nor negative correlations can occur without the presentation of shocks, but I see no need to "account for the derivation of positive from negative reinforcement" (p. 337). I do not conceive of danger as primary and of safety as something that is secondary to or derivative from danger; I think of safety as a behavioral function that develops concurrently with danger when the animal is exposed to a set of correlations between shocks and previously neutral stimuli. Within this context, I could ask in parallel fashion why a stimulus that is positively correlated with shock becomes aversive or negatively reinforcing, but it is not clear to me that that would be a meaningful question or that it has a meaningful answer. I have the same reaction to a stimulus that is negatively correlated with shock. I am content with a description of the functional relations involved.

#### BAUM

When I was a graduate student, my mentors (F. S. Keller and W. N. Schoenfeld) impressed on me the importance of basing my conclusions on empirical data, whatever the theoretical context may have been within which they were gathered. Accordingly, throughout my target article (Dinsmoor, 2001), I tried to hew as closely as possible to concrete empirical findings, frequently citing data embedded within alien vocabularies. With only a very few exceptions, these findings have been ignored rather than directly addressed by either Baum (2001) or Hineline (2001). Baum, for example, dismisses a substantial body of research with the comment that these experiments "depend on failure of discrimination. . . . There are many ways to confuse a rat" (p. 340).

For the most part, both Baum (2001) and Hineline (2001) have responded to the empirical data by turning to more general and more abstract considerations. In Baum's view, for example, the successes of molar theory can be ascribed to its being "more productive" (p. 340), but the successes of two-factor

theory must be attributed to its being irrefutable, meaning untestable (p. 339). I am said to be defending "19th-century atomism against the onslaught of a new conceptual framework. . . . The molar view of behavior arose in response to the demonstrated inadequacy of explanations based on contiguity" (p. 338). (What demonstration?) "The conflict is paradigmatic" (p. 340). Copernicus is contrasted with Ptolemy. (Which of our theories is systematic and which invokes one or more epicycles?) It is difficult to determine how statements at this level of generality bear on the problems at hand, but when Baum's paradigms clash with my facts, it is the facts that must prevail.

Similarly, Hineline's (2001) commentary begins with a discussion of molar theory as a whole and his views on overlapping time scales. I am said to be besotted by contiguity and insensitive to context. Much of what Hineline says simply repeats comments that he has published elsewhere and that have no apparent relation to the issues between us. Like Baum, he fails to come to grips with the data.

Baum (2001) maintains that the molar view is required to investigate the interaction between magnitude and delay of reinforcement. Why? As noted in my target article (Dinsmoor, 2001), the delays are well within the time scale of standard conditioning theory (see Logue, 1995).

Baum (2001) also notes that I ignored his earlier explanation (Baum, 1973) of the inverse relation between the delay and the effectiveness of a reinforcer as resulting from a reduction in the temporal correlation between responses and consequences. That is true. I did not know at the time I wrote my target article that it would be the subject of a commentary by Baum or I would have anticipated his reaction.

The reason I ignored his explanation is because I did not consider it relevant to my argument. As Baum (2001) himself acknowledges, "Events that will occur sometime in the future . . . affect present behavior less and less the more remote they are" (p. 340). That passage reads to me like an endorsement of temporal proximity as a general principle. The only point that I was trying to establish with the data on delay of consequence was that under most circumstances the long-term

correlations to which Baum and Hineline (2001) appeal cannot be effective. Unless the declines in efficacy that I cited can be shown to result specifically from a deterioration in those long-term correlations, then the cause does not matter. Their consequence is not effective. In point of fact, the delays that I cited in my target article were of short duration. The declines in the efficacy of a consequence as a function of time were extremely steep for the first few seconds, but leveled out as they approached zero. In other words, the reduced correlation between response and reinforcer was at the short end of the time scale—Skinner's contingency—but in most of those experiments the programmed reinforcers were eventually delivered. On the time scale to which the principle of shock-density reduction and the molar principle of reinforcement in general make their appeal, the correlation was not impaired. Given, then, the empirical observation that reinforcing events delayed more than, say, a minute or so are rarely efficacious—the slope of the function becomes so gradual that it is impossible to set a precise limit—correlations that emerge only some time after the occurrence of the response cannot ordinarily provide the reinforcement necessary for the acquisition and maintenance of avoidance behavior.

Finally, it becomes clear from Baum's (2001) commentary that by "hypothetical entities" he means something very different from the meaning attached to that expression by other writers. He is alluding not to hypothetical constructs such as fear or expectancy but to descriptive terms like *aversive*, *safety*, and *conditioned reinforcement*. These terms do not refer to anything unobservable but are used throughout the conditioning literature as reasonably well-defined labels for important functional relations.

#### HINELINE

To the charge of attacking a straw man (Hineline, 2001), I must plead not guilty. What I said in my introduction was carefully worded: "It is frequently assumed that these two theories are mutually exclusive and that shock-frequency or shock-density reduction is accepted behavior-analytic doctrine" (Dinsmoor, 2001, p. 311). Often, two-factor theory is not even mentioned, which was the prob-

lem that originally led to my writing the target article. However, I will not cite a list of examples for this characterization of the literature, because most of them are secondary sources and singling the authors out as individuals would suggest an attempt to chastise them for what is, after all, a common shortcoming. Perhaps it is mainly secondary sources, too, that have overstated the claims of molar theory, but in his *Psychological Review* article Herrnstein (1969) did conclude that "CS termination is neither necessary nor sufficient and . . . the reduction of aversive stimulation is probably both" and that "The theme of the current paper is that the reinforcement for avoidance behavior is a reduction in time of aversive stimulation" (p. 67). Furthermore, the very name *single-process theory* suggests that it was proposed as a substitute, rather than a supplement, for two-process theory. I carefully refrained from the assertion that Hineline himself believes two-process theory to have been supplanted by shock-frequency reduction. I had been uncertain about that, and I welcome his explicit statement to the contrary.

I made no attempt to "[portray] nonmolecular theory . . . as synonymous with the principle of shock-frequency reduction" (Hineline, 2001, p. 343). It is the proponents of molar theory who have tried to broaden the issue. I assume that my critique of shock-frequency reduction has implications for molar theory more generally, but I did not tackle that topic as such. In writing the target article my purposes were to detail an updated version of two-factor theory, to demonstrate that this theory integrated a wide variety of findings usually either neglected or treated as discordant, and to dispose of shock-frequency reduction as an alternative source of reinforcement. A more general critique of molar theory, the analysis of behavior on fixed-ratio schedules requested by Baum, or the discussion of multiscaled analyses that Hineline proposes at the end of his commentary would have taken me far beyond those objectives.

I contested Hineline's (1981, p. 228) use of one particular strand of molar theory—the molar principle of reinforcement—for one particular reason: because he brought it up as a defense against my criticism of the role of shock-frequency reduction in the reinforcement of avoidance (Dinsmoor, 1977,



2001). Most of the examples of molar theory cited by its proponents in the present exchange seem to me to be irrelevant to that issue, and as a whole, molar theory seems to me too broadly and too amorphously defined to be evaluated as a homogeneous entity.

For example, Hineline (2001) treats the concept of safety—which Baum (2001, p. 340) considers a hypothetical entity comparable to phlogiston—as “inherently a molar concept” (p. 344; see also p. 345). My analysis of the data collected by Mellitz et al. “also is an appeal to molar variables” (p. 345). Demonstrations that changes in the schedule of reinforcement in one component of a multiple or a chained schedule may affect behavior in another component (see Krasnegor, Brady, & Findley, 1971; Sidman & Boren, 1957) are also cited as examples of molar theory (Hineline, p. 346) but they tell us little about the reinforcement of avoidance.

Similarly, in Baum’s (2001) commentary, a molar perspective is said to be required to examine the trade-off between the magnitude and the delay of the reinforcer or punisher that is examined in experiments on self-control (p. 340). (Temporal proximity is not relevant?) “The molar view casts the effects of delay into questions for research” (p. 340). “Research on timing, delayed discrimination, and temporal discounting all come together to focus on this problem” (p. 340).

According to Bersh’s (2001) commentary, averaging the delays to reinforcement following the effective response and comparing them to the delays following other responses “is hardly . . . molecular” (p. 348). Similarly, reference to “the interresponse time, by definition a relation between successive responses . . . is again not entirely molecular” (p. 348). “Rate [of responding] is a molar construct” (p. 349). Molar theory reminds me of the British Empire during the height of its colonial expansion. The molar commentators seem to be planting the Union Jack on a wide array of territories stretching around the globe and claiming them as their own. To paraphrase the British jingoists, the sun never sets on molar theory.

The only way I see by which these claims can be linked to the present discussion must be that if I accept the cited findings as valid data, I am accepting a molar interpretation of them; that if I am accepting a molar inter-

pretation of specific findings I am accepting molar theory as a whole; and that if I am accepting molar theory as a whole, then I must accept the molar (i.e., shock-reduction) theory of reinforcement. I do not find that logic acceptable, and without it I do not find these examples of molar theory relevant.

Finally, to illustrate overlapping time scales, Hineline (2001) turns to everyday examples of human behavior. This is legitimate enough as a means of illustrating a concept, but it should be kept in mind that anecdotal accounts of human behavior have always reflected far different time horizons than laboratory accounts of the behavior of rats and pigeons. As yet, we have little trustworthy knowledge of how these anecdotal relations arise.

### BERSH

Bersh (2001) agrees with Hineline that molar theorists are inclusive, incorporating the two-factor theory of avoidance conditioning rather than proposing that long-term shock-frequency reduction is a *substitute* or *replacement* for that analysis. “Although a conditioned aversive stimulus acquires its aversive properties through respondent conditioning, and its termination functions as negative reinforcement,” he goes on to say, “this by no means rules out the consideration of a warning signal as a discriminative stimulus” (p. 348). No, that certainly was not my intent. As was stated in my target article, I had suggested many years ago that the signal could serve a discriminative function (Dinsmoor, 1952, 1954). What is more, in responding to Hineline’s writings I had found it necessary to assert the reverse of Bersh’s statement: that if a stimulus served a discriminative function, that by no means ruled out its consideration as an aversive stimulus (Dinsmoor, 2001, p. 325).

As might be expected on the basis of his graduate training with W. N. Schoenfeld and F. S. Keller, Bersh (2001) does scrutinize the empirical data. He believes, for example, that certain results obtained by Gardner and Lewis (1977) provide support for Hineline’s hypothesis that shock-frequency reduction over a temporally extended period is a sufficient condition for reinforcement. In their Experiment 2, Gardner and Lewis used a seemingly simple but highly confounded procedure. A

peck on the key during the originally imposed condition changed exteroceptive stimuli (key color and clicks) and simultaneously programmed a future change in the schedule of shock delivery. The experimental manipulation was to continue the variable-time 15-s schedule of shocks imposed prior to the stimulus change, for 0, 1, 2, 3, or 4 more shocks before the remaining shocks were canceled. After that, no more shocks were delivered during the remainder of a 2-min period of alternative stimulation.

Bersh's (2001) conclusion is in agreement with those reached by the original authors. However, the experiment itself was flawed. For example, the shocks that continued into the alternative condition were added *ad seriatim* from the beginning of that condition. This means that the number of shocks delivered (categorized by Gardner & Lewis, 1977, under the heading of frequency) was highly correlated—confounded—with the length of time before the series terminated (categorized as delay). Also, these shocks were delivered in the presence of the alternative stimulus complex produced by the response. The change in stimulus is important because stimulus mediation is the heart of two-factor theory. Bersh correctly identifies the role of the altered stimulus as that of a partial safety signal. The change from the originally imposed stimulus to an alternative stimulus correlated with termination of the series of shocks must have encouraged the subject to continue responding.

To Gardner and Lewis (1977), the fact that pecking was usually maintained despite delivery of the first two shocks following the response ruled out delay as a factor in interpreting their results. It is not clear, however, why the authors drew an arbitrary distinction between the second and the third shock in the series.

The shocks following the peck were not distributed evenly over the 2-min period but were bunched together toward its beginning. This is not the correlation over an extended period of time to which Himeline made his appeal (e.g., Himeline, 1981, p. 227). When only one shock was delivered following the response, it came after an average delay of about 7.5 s. When two shocks were delivered, the second one must have come about 22.5 s after the response. In two of five instances,

adding that shock suppressed the response. Note further that there was a dispersion around the mean delay, so that the second shock sometimes arrived substantially earlier. When three shocks were delivered, the third shock must have come about 37.5 s, on average, after the response, and this shock suppressed responding in the third and fourth assays and substantially weakened it in the fifth (see Gardner & Lewis, 1977, Figure 5). Even when four shocks were required, the maximum and wholly effective number, most of them must have arrived within the 1st minute of the 2-min period. This is well within conventional (molecular) temporal limits.

Lambert, Bersh, Himeline, and Smith (1973) pitted a delayed group of five shocks—delivered at 1-s intervals—against an immediate single shock. To interpret their data, I do not need to assume that “an overall reduction in aversiveness reinforces the response” as Bersh (2001, p. 350) maintains. I can be more specific: As in studies classified under the heading of self-control, an interaction between the delay and the magnitude of the shock determined the outcome (see Deluty, 1978). There is a large literature on self-control (Logue, 1995).

The possibility that an experimental design similar to that of Dinsmoor and Sears (1973) could be used to substantiate the efficacy of terminating a warning signal as reinforcement (Bersh, 2001)—a conclusion that is not in question—does not in any way weaken the conclusion that production of a safety signal is a reinforcer. Contrary to Bersh's argument, the two operations would remain distinguishable, independently verified.

I keep being accused of being a molar theorist. Bersh (2001) suggests that, like shock-frequency reduction, rate of responding, which I often have used as a dependent variable, is in itself a molar construct. Be that as it may, my first criticism of shock-frequency reduction was that it cannot be localized in time following a specific item of the subject's behavior, like the usual pellet or tray of grain, and therefore that it cannot participate in the usual temporal relation (contingency) between response and reinforcer. My second criticism was that there are no empirical data that specifically or selectively support the role of a consequence correlated only over an extended period of time. In fact, substantially

delayed reinforcement is usually ineffective. These are criticisms that apply to shock-frequency reduction as a reinforcer but have no relation to rate of responding as a dependent variable. I do not suffer from a general phobic reaction to all fractions in which the number of events is the numerator and time is the denominator.

### BRANCH

Branch (2001) is somewhat supportive of the two-factor approach to avoidance, but he takes issue with several aspects of my argument. First, he rejects my analysis of the Sidman (1962) data that initially suggested long-term shock-density reduction as the source of reinforcement for the behavior that avoids the shock. He states that my analysis "hinges crucially on the assumption that the rat's behavior was sensitive to the scheduled source of shocks (i.e., in lay language, that the rat could tell from which avoidance schedule the shocks came)" (p. 352). That is not correct. It is the behavior analyst who needs to draw such a distinction, in order to analyze the data most effectively, but the rat has no need to do this and presumably cannot do any such thing. If I had implied that it did, other commentators would have noted the error and would have pounced on it. In the passage that Branch quotes in support of his interpretation, he substitutes "a rat" (in brackets) for the pronoun "it." From the sentence that precedes that passage, however, it should be clear that the word "it" refers to "the safety signal formulation," not to the rat.

In his comments on the Feild and Boren (1963) experiment, Branch objects to my characterization of third-order conditioning as very weak, because he believes two-factor theory requires that form of mediation to explain the aversive character of early stimuli in sequences leading to shock. I have not collected any empirical data designed to determine the precise limits of first-order conditioning under the parameters set by Feild and Boren, but I think they must be well within the general zone of uncertainty for this function. I do not think that first-order conditioning is ruled out by any temporal values I have suggested at other points in my discussion. When no stimuli were provided, the rats rarely stopped pressing, even though the shock

was relatively distant in time. They had no basis for a discrimination. When correlated stimuli were provided, however, those that preceded the shock by as little as 50 to 60 s were evidently sufficiently remote from the shock to serve as safety signals. In the face of a visual and auditory safety signal already provided by the experimenter, additional safety signals representing still greater temporal distance from the shock or induced by the response itself become redundant. That is why the subject stops responding before reaching the maximum temporal distance from the shock. (Incidentally, this phenomenon does not require a free-standing ad hoc principle, as proposed by Hineline, 1984, 2001. It is inherent in the safety-signal analysis.)

Branch (2001) is also bothered by the failure of experimental subjects to postpone warning signals. When I refer to a stimulus as *aversive*, however, I mean simply that its termination is reinforcing, as in escape training. It is a negative reinforcer. "[Defining] the warning stimulus as aversive" does not automatically imply that it is "something that will support its avoidance" (p. 353). Implicit in my definition is the stipulation that the aversive stimulus must be replaced by some other state of affairs for the operation to be effective. Under a simple free-operant shock-postponing procedure (e.g., Sidman, 1953a, 1953b), that replacement occurs. The animal is sometimes shocked in the presence of the preresponse stimulation but is never shocked just after a response; when the animal responds, then, the conditioned aversive stimulus is replaced by the response as a safety signal. When an exteroceptive warning signal is added to the procedure, a response in the presence of that warning signal may, depending on the contingencies, constitute a safety signal, but a response in the absence of that warning signal does not, even if the warning signal continues to be absent following the response.

I do not understand how Branch (2001) thinks that some method of averaging could account for the results obtained by Hineline (1970) or Gardner and Lewis (1976). In both cases, the frequency was calculated by dividing the number of shocks by the period of time during which they were delivered. Also, I do not understand the reasoning by which he concludes that if short delays are dispro-



portionately influential with positive reinforcement then long delays are likely to be disproportionately influential with negative reinforcement. I would argue that the same rule applies to both functions.

In my target article, I offered a logically plausible alternative to Bolles' (1978) widely cited theory of species-specific defense reactions as an explanation for differences in the speed with which avoidance learning occurs with different response topographies. I suggested that any such differences might instead be a function of the discriminability of the feedback to the animal. However, I agree with Branch that that hypothesis will not be easy to test.

#### MICHAEL AND CLARK

Although they believe that the safety-signal component is not a critical part of my argument, Michael and Clark (2001) are largely in agreement with two-factor theory in general and with most of my analyses of the literature on avoidance.

Their suggestion that the results obtained by Dinsmoor and Sears (1973) can be attributed to a generalization decrement from the contextual stimuli of the conditioning chamber to those same stimuli plus tone (p. 355) rests on a chain of logic that is fairly complex. Although their interpretation might be used to explain the reinforcing effect of the 1,000-cycle tone—a change from the maximally aversive situation when the tone is absent—it does not seem to me to explain the gradient of generalization obtained for a range of tones of differing frequency. These tones do not differ from the situation without tone any less or any more than does the tone of 1,000 cycles used during the training. Nor, as far as I can see, do compounds including these various tones differ any more or any less. Changing the frequency of an orthogonal tone is not equivalent, in my opinion, to adding a flickering light to the contextual stimuli. In their account, where does the gradient of generalization come from?

Their quantitative analysis of Sidman's (1962) two-lever experiment points out that the total frequency of shock was indeed lower when the animal persistently pressed the lever controlling the shorter shock-shock and response-shock intervals than when it

pressed the lever controlling the longer intervals. This was the difference in consequence to which Sidman appealed in interpreting his results, and it led to his suggestion that it was a long-term reduction in the overall density of shock that was responsible for the reinforcement of avoidance behavior in general. However, for reasons that I stated elsewhere in my target article, I doubt that interpretation. The point I was making in discussing Sidman's experiment was that although it occurred, the reduction in total number of shocks was not the relevant factor; as an alternative, I suggested that the difference between shock density immediately before and immediately after an occurrence of the critical response, mediated by the stimulus change from absence to presence of the response, was what provided the reinforcement. That alternative was consistent with two-factor theory and unlike total shock frequency provided a short-term, selective contingency or correlation between the shock schedule and the response.

#### BARON AND PERONE

In their commentary, Baron and Perone (2001) are sympathetic to a two-factor account. Although Baum (2001) argues that a molar approach leads to a more detailed examination of the data, they suggest just the opposite: "[Molar accounts] may have the unintended consequence of undermining the search for the variables that control specific instances of behavior" (p. 358). They point out that the very definition of avoidance requires reduction in the overall density of shock—if no such reduction occurs, the behavior is not categorized as avoidance—and suggest that density reduction "is more a restatement of the behavior in need of explanation than a specification of the variables that control it" (p. 358).

They make explicit a point that was only implicit in my target article: that a correlation between rate of responding and rate of reinforcement or frequency of shock does not of itself demonstrate causation. It is necessary to isolate the critical variables. That, of course, is the task that Skinner (1938/1991) set in his first book, subtitled *An Experimental Analysis*.

Baron and Perone (2001) examine the experiment by Herrnstein and Hineline (1966),

which those authors submitted as their evidence for frequency reduction as a reinforcer. In my 1977 review I had argued that the Herrnstein–Hineline experiment did not provide any support for their hypothesis, because the data were entirely compatible with two-factor theory: “The rate of pressing was a positive function of the probability of shock prior to the response and an inverse function of the probability of shock during the period immediately following the response” (Dinsmoor, 1977, p. 90). In addition, Baron and Perone note that the time scale in that experiment was more in keeping with a molecular than with a molar perspective. As an alternative to the Herrnstein and Hineline experiment, they cite Perone and Crawford (1999) for a detailed discussion of related work that they consider more suitable for a test of Herrnstein and Hineline’s hypothesis but that also failed to provide it with empirical support.

In addition, Baron and Perone (2001) raise an issue that I did not address in my target article—“the Pavlovian responses that may be induced by aversive stimuli and inhibited by safety signals” (p. 360). I did not want to open that can of worms, because it is not necessary to my argument. I note, by way of comparison, that standard accounts of conditioned positive reinforcement do not refer to any Pavlovian responses. I see no reason why the case for conditioned negative reinforcement should be any different. The presence of such responses in other people’s accounts may be a vestigial legacy from early drive-reduction theories of avoidance (e.g., Mowrer, 1950) or from subsequent clinical writings.

Toward the end of their commentary, Baron and Perone (2001) express regret that so few data have been collected in recent years on such an important topic as avoidance. Little work has been done on escape or punishment (see Dinsmoor, 1998) either, and to me the time course of this development suggests that the dearth of recent research employing aversive stimuli results from the chilling effect exerted on scientific inquiry by the campaign for animal rights.

#### WILLIAMS

I am encouraged to find that Williams (2001) shares my doubts as to how long-term

shock-frequency reduction per se can make contact with the subject’s response and my preference for an account that centers on the “reduction in aversiveness caused by the transition in the stimulus complex” (p. 362) from before to after the response.

Although I am somewhat reluctant to appeal to the clinical literature for empirical support for two-factor theory, as Williams does, I am happy to be in a position to extrapolate in the opposite direction (see Branch & Hackenberg, 1998; Dinsmoor, 1991; Donahoe & Palmer, 1989). It seems to me that the data obtained in the laboratory from nonhuman animals have much greater application to human behavior in everyday situations than the inferences developed in the clinical setting have to the understanding of the behavior of nonhuman animals in the laboratory. It is the implications of laboratory data for life outside the laboratory that have motivated a substantial part of my work in conditioning; and it is in part the disconnection I see between the single-process approach and the clinical applications of the experimental findings that has motivated my attempt to divert behavior analysis from what I consider to be a theoretical cul-de-sac. To the extent that it has cast doubt on two-process theory in behavior-analytic circles, single-process theory has tended to distance our findings from the attention of the clinical psychologist and members of the general public.

#### CONCLUSIONS

The format of target article, commentaries, and reply tends to focus attention on the theoretical dispute between the two-process and single-process approaches to avoidance. Debate is important, and I still do not see any evidence that selectively favors a temporally extended reduction in the density of shock as a reinforcer of avoidance. If there is a major long-term contribution contained in my target article, however, it probably resides in the conclusion that the avoidance response is itself a stimulus and that by its negative correlation with the concurrent incidence of shock, that stimulus becomes an automatic reinforcer. A large and varied array of converging evidence was cited in support of that conclusion.

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<sup>1</sup> Feild is misspelled as Field in the article referenced here.

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